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XV.

ON THE PRESENT STATE OF THE QUESTION OF STANDARDS OF LENGTH.

By W. A. Rogers.

Presented April 14, 1880.

It is not my intention in this paper to enter into a minute account of the construction and comparison of the various standards of length which have been made the basis of measurements, either in trigonometrical surveys of the earth's surface, or in more strictly physical investigations. Many of these possess a certain historical interest, even when they have but little inherent value. For information of this kind, the reader is referred to the references at the end of this paper.

I shall confine myself to a consideration of those standards of length which are in actual use, and which have the authority and sanction of either national or international law.

Three natural units have been proposed as the basis of a standard of length, as follows: —

I. The length of a pendulum beating seconds in a vacuum at the level of the sea in the latitude of London.

II. One ten-millionth part of a quadrant of the earth's surface.

III. The length of a wave of light of given refrangibility.

It is generally supposed that the yard of Great Britain was founded upon the first of the natural units named, but it will be seen from the act of Parliament legalizing the standards prepared by the Royal Commission, signed June 17, 1824, that the reference of the standard of length to this unit refers to its restoration in case of loss or destruction, and not to its original construction. Notwithstanding many experiments were made at this time by Kater and others for the purpose of ascertaining the length of the standard expressed in terms of the length of a seconds pendulum, the yard actually legalized was constructed from Graham's scale by Bird, in 1760. It is marked, "Standard 1760."

The reason assigned for selecting Bird's "Standard 1760" was its close agreement with Shuckburgh's scale (0 – 36$^{\text{in.}}$), made by Troughton in 1798, with which the pendulum and the meter had been compared, and of which a fac-simile was known to exist at Geneva. It is interesting to note, in passing, that, previous to Shuckburgh, all transfers were made by means of beam compasses, and all comparisons were made in the same way. The first use of optical means of comparison must be credited to Troughton in 1798, when he transferred Bird's scale for Shuckburgh. The following is the act legalizing the standards: —

"SECTION I. Be it enacted . . . that from and after the first day of May one thousand eight hundred and twenty-five, the Straight Line or Distance between the Centres of the Two Points in the Gold Studs in the Straight Brass Rod, now in the Custody of the Clerk of the House of Commons, whereon the Words and Figures 'Standard Yard 1760' are engraved, shall be and the same is hereby declared to be the original and genuine Standard of that Measure of Length or lineal Extension called a Yard; and that the same Straight Line or Distance between the Centres of the said Two Points in the said Gold Studs in the said Brass Rod, the Brass being at the temperature of Sixty-two Degrees by Fahrenheit's Thermometer, shall be and is hereby denominated the 'Imperial Standard Yard.' . . . .

"SECTION III. And whereas it is expedient that the said Standard Yard, if lost, destroyed, defaced, or otherwise injured, should be restored to the same Length by reference to some invariable natural Standard; And whereas it has been ascertained by the Commissioners appointed by His Majesty to inquire into the subject of Weights and Measures, that the said Yard hereby declared to be the Imperial Standard Yard, when compared with a Pendulum vibrating Seconds of Mean Time in the Latitude of London in a Vacuum at the Level of the Sea is in the proportion of Thirty-Six Inches to Thirty-Nine Inches and one thousand three hundred and ninety-three ten-thousandth Parts of an Inch: Be it therefore enacted and declared, That if at any Time hereafter the said Imperial Standard Yard shall be lost or shall be in any Manner destroyed, defaced, or otherwise injured, it shall and may be restored by making a new Standard Yard, bearing the same Proportion to such Pendulum as aforesaid as the said Imperial Standard Yard bears to such Pendulum."

On the 16th of October, 1834, both houses of Parliament were consumed by fire. The bar of 1760 was recovered, but in a condition which rendered it useless as a standard, one of the gold plugs having been melted out. It now became necessary to decide whether it should be restored in accordance with the act of June 17, 1824. Since the passage of the act, it had been shown that all the elements which were defined in the act for restoration were subject to some doubt. Dr. Young had shown that the reduction to the level of the sea was doubtful. Both Bessel and Baily had shown that the reduction for the weight of the air was erroneous. Baily had thrown doubt upon the estimated specific gravity of the pendulum employed, and upon the accuracy of the agate planes, while Kater himself showed that sensible errors had been introduced in comparing the pendulum with Shuckburgh's scale.

In view of these facts, all attempts to restore the lost standard in accordance with the act of June, 1824, were abandoned. Instead, it was decided to attempt the restoration of the lost standard from the various standards which had been previously compared with it. There were available for this purpose, —

Shuckburgh's scale ($0 - 36^{\text{in.}}$);

Shuckburgh's scale, with Kater's authority;

The yard of the Royal Society, constructed by Kater;

The Royal Astronomical Society's brass tubular scale;

Two iron bars, marked A_1 and A_2, belonging to the Ordnance Department, and preserved in the office of the Trigonometrical Survey.

The restoration of the standard was placed in the hands of Sir Francis Baily. At his death, in August, 1844, he had done little more than complete the provisional inquiries required before attempting the final construction. He had, however, after many experiments, decided upon the material of which the new standard should be composed. It has since his time been known as Baily's metal. Its composition is, copper 16, tin $2\frac{1}{2}$, zinc 1.

Upon the death of Mr. Baily, the work of restoration was committed to the Rev. R. Sheepshanks. Sir George Airy, in his account of the construction of the new national standards of length, has given an exceedingly interesting communication from Mr. Sheepshanks (Philosophical Transactions, 1857, p. 661), detailing the means he employed for the restoration of the lost standard. He first constructed a brass bar, as a working standard. This "brass bar 2" was compared with all the standards which Mr. Sheepshanks considered properly available for this purpose, with the following results: —

Inches.
Brass bar 2 = 36.000280 by Shuckburgh [$10-46^{\text{in.}}$].
= 36.000084 by Shuckburgh [$0-36^{\text{in.}}$, Kater].
= 36.000229 by Kater's scale.
= 36.000303 by A_1 compared in 1834.
= 36.000275 by A_2 compared in 1834.
Mean, 36.000234

He assumed: —

Brass bar 2 = 36.00025 inches of the lost imperial standard at 62° Fahrenheit.

It is stated both in Appletons' and in Johnson's Encyclopædias that the yard of the Astronomical Society is the principal authority upon which the new standard rests. It will be seen from the above that this statement is erroneous.

The Imperial Standard Yard, known as "Bronze 19," or, according to the new nomenclature, as No. 1, was constructed according to this equation. It is made of Baily's metal, and has the following dimensions, viz.: —

Length = 38 inches.
Width = 1 inch.
Depth = 1 inch.

Gold plugs are inserted in wells sunk one half of the depth of the bar. The graduations are on these gold plugs.

Through the kindness of Mr. Chaney, the Warden of the Standards, I have recently had the pleasure of assisting him in comparing my own working standard yard with this bar. I have never seen lines better adapted to exact measurements. They have remarkably smooth edges, and are about one three-thousandth of an inch in width. Even for the most rigid scientific investigations, they are in every respect of unrivalled excellence.

Bronze No. 1 is the national standard, and is kept in what is known as the "Strong Room" of Old Palace Yard. Besides this bar, four Parliamentary copies were made, of which one copy is kept at the Royal Mint, one is in charge of the Royal Society, one is immured in the new Westminster Palace, and the other is kept at the Royal Observatory, Greenwich. Forty copies were prepared on Baily's metal. Of these only two are exactly standard at 62°, viz. Bronze 19, and Bronze 28, which is kept at the Royal Observatory as an accessible representation of the national standard. All the other copies have an equation with respect to No. 1; but instead of giving this equation, the degree at which each bar is standard is given. These bars have all been distributed among different governments.

The standards prepared by Mr. Sheepshanks were legalized by act of Parliament passed July 30, 1855.

Two platinum bars — one a line-measure and the other an end-measure — form the basis of the metric standard of length at present in use in Great Britain. These bars have the following dimensions: —

	Line-Meter.	End-Meter.
Length,	41.0 inches.	39.37 inches.
Breadth,	1.0	1.00
Thickness,	0.211	0.287

The line-meter has the words "Royal Society, 45," engraved on the under side. The defining lines run nearly across the face of the bar, and there is no cross line to indicate the exact points from which measures are to be made. Arrows, arbitrarily placed, now indicate the points from which all the later measures have been made. The lines on this bar have become so obliterated that it is found impossible to see them with the method of illumination formerly in use. At the request of Mr. Chaney, I employed Mr. Crouch, of London, to construct two new objectives for the microscopes of the comparator, to which were adapted two of Tolles's interior illuminators for viewing opaque objects. The magnifying power was increased between five and six times, and the illumination was so much improved that there was no difficulty whatever in seeing the lines of this bar.

The end-meter has the words "Mètre à Bouts" engraved on one side, and the words "Fortin à Paris, Royal Society 44," on the other side. At the present time, this standard is not in a condition to admit of accurate measurements. The edges of the end surfaces are indented, and there is a raised burr on one end. These bars, together with the original bars compared by Hassler in 1832, are the only recognized standards which have ever been compared with the Meter of the Archives. They form, therefore, the real basis of our present knowledge of the relative length of the meter and of the yard. I shall presently recur to this important matter.

The line-meter was transferred by Troughton and Simms to a bar of Baily's metal in 1869. Kater's reduction to the Meter of the Archives was applied in the transfer. The Imperial Yard was also transferred to the same bar. The lines are drawn upon gold plugs, which are not, however, inserted in wells. At the distance of 39.37 inches from the first plug the "meter plug" is inserted. This meter is the one from which Chisholm obtained his value of the equation between the meter and the yard. It is now the working standard meter of the Exchequer.

A second natural unit, viz. one ten-millionth part of a quadrant of the earth's surface, was adopted at the close of the last century as the basis of the metric system. In March, 1791, a committee of the Institute of France, consisting of fifteen members, including Borda, Lagrange, Laplace, Mongé, and Condorcet, recommended this unit as a standard of linear measure. It is interesting to note that the Commission was largely international in its composition. Besides the members of the Institute, there were delegates from the Republic of the Netherlands, from Sardinia, Denmark, Spain, Tuscany, the Roman Republic, the Cisalpine Republic, the Ligurian Republic, the Swiss Confederation, and from Piedmont. Their report was sanctioned by the Assembly, and an arc of the meridian passing through Paris, and extending from Dunkirk to Barcelona (stations differing about 10° in latitude), was measured with great care by Méchain and Delambre. By combining the results of this survey with arcs previously measured in Peru and Sweden, the length of a meridional quadrant passing through Paris was ascertained. For the measurement of base lines, two separate toise end-measures were employed, each assumed to be equivalent to the toise of Peru. From these standards, four iron bars were prepared, having their ends carefully ground and polished until they were exactly comparable with each other, and until each had the required length. One of these original bars, bearing upon it the stamp of the Commission, is now in the possession of the United States Coast Survey. It is understood to be the only copy now in existence. One bar was chosen as the standard of France, and from it was constructed the platinum "Mètre des Archives." Two similar meters, made at the same time and in the same manner, are now in existence, viz. the "Mètre du Conservatoire," and the "Mètre de l'Observatoire." All of these meters are end-measures, and have about the same dimensions as the English platinum meters already described. The equation between the "Mètre des Archives" and the "Mètre du Conservatoire" is small. That between the "Mètre des Archives" and the "Mètre de l'Observatoire" is not well determined. Only one line-measure is known to have been made at the time of the construction of the end-measures. This was transferred from the "Mètre de l'Observatoire," probably about two years after the adoption of the "Mètre des Archives" as a standard.

In 1870, a commission was formed at Paris, under the title "Commission International du Mètre," for the purpose of settling all doubts in regard to the value and permanence of the unit bases. In 1874, this commission decided to maintain at Paris an "International Bureau of Weights and Measures, to be supported by *pro rata* contributions from all the signing powers, and charged with the care of the prototype standards, and with the duty of constructing and verifying copies of those standards, not only for the powers interested, but for other governments, for corporations, and even for private individuals who should apply for them, and who should be willing to pay the expense attending their construction and comparison."

It is not necessary to record here the considerations which led the Commission to abandon all attempts to establish a standard which should conform to the natural unit, in accordance with which the original meters were constructed. It is sufficient to say that the Commission decided that the "Mètre des Archives" shall be recognized and perpetuated forever as the true base of the measure of extension, without regard to the doubtful questions which have been raised concerning its correspondence with its theoretical value. The first resolution of the "Convention du Mètre," signed May 20, 1875, reads as follows: "For the execution of the international meter, the Mètre des Archives, in the state in which it is found, is taken as the point of departure."

The Convention adopted twenty-one resolutions respecting the meter, of which the most important are the following: —

"IV. Though deciding that the new international meter shall be a line-meter, of which every country shall receive identical copies, constructed at the same time as the line prototype, the Commission will nevertheless undertake to construct a certain number of end-measure standards for the countries which shall express a desire for them, and the equations of these end-meters with respect to the new line prototype shall be determined with equal care by the International Commission.

"V. The international meter shall be of the length of the meter at 0° C.

"VI. In the fabrication of the meters, an alloy shall be employed, composed of 90 parts of platinum and 10 parts of iridium, with an allowance of two per cent variation, more or less."

"IX. The bars of platinum-iridium, upon which the lines are to be traced, shall be 102 centimeters in length, and their transverse section shall be represented by a model described in a note by M. Tresca."

"XIII. The method of M. Fizeau shall be employed in determining the expansion of the platinum-iridium, of which the meters are formed.

"XIV. The prototype shall be submitted to the best possible process for the determination of the absolute coefficient of expansion of the whole meters. These measures shall be made separately, at least at five different temperatures comprised between 0° and 40° C."

"XVII. The comparisons shall be made by immersing the new standards in a liquid and in air, but with the reservation that the standard of the Archives shall not be immersed in any liquid until the close of the operations.

"XVIII. The tracing of the line-meters, and their first comparison with the 'Mètre des Archives,' shall be effected by the process of M. Fizeau."

In order to gain a clear understanding of what has been accomplished in accordance with these resolutions, it is necessary to distinguish between the operations of the French section of the Commission and those of the International Bureau. The construction of the prototypes from the "Mètre des Archives" was naturally committed to the French section, the primary standard being the property of the French government, the responsible authority for its preservation in a state of perfect integrity.

The prototypes thus prepared, after delivery to the International Commission, and after their formal acceptance by the Commission, are to be subjected to the various tests provided for by the articles of the convention. Finally, either one of these prototypes, or, perhaps, the mean of two or three of them, will be officially declared to be the true and final base of reference in all measurements of linear extension.

The International Bureau is now prepared for its share of the work. The buildings are completed, the instruments of comparison are in position, and nearly all of the provisional investigations required have been completed. The present delay must be charged to the unfortunate dead-lock between the Commission and the French section, the Commission refusing to accept the prototypes prepared by M. Tresca, mainly on the ground that the material of which the standards are made is not, in its present condition, a pure alloy of platinum and iridium, but that it contains, as is asserted, nearly two per cent of iron.

The International Bureau at the present time has no standards which have been compared directly with the "Mètre des Archives." It has three provisional meters, all of which have been indirectly compared with the primary standard. It has a meter made by Repsold, one made by Hermann and Pfister, of Berne, Switzerland, and one made by Brunner Frères, of Paris, which is presumably a copy of the line-meter derived from the meter of the Observatory. The meter by Brunner Frères, the tracings on which are very good, is taken as the present provisional standard.

The buildings of the International Bureau are situated on the summit of a high hill at Breteuil, on the direct road from Paris to Versailles. The location is an admirable one in every point of view. During my recent visit to Paris, a very kind letter of introduction from Professor St. Claire Deville to Dr. Pernet, the Director of the Bureau, led to a very interesting visit to this establishment. There are three principal observing-rooms, perhaps eighty meters square. The walls are composed of corrugated metal, the waves running longitudinally. The rooms are lighted by a circular skylight only.

In one room is found the apparatus for comparing standards of weight, under the charge of Dr. Marek. There is here a marvellously perfect apparatus for weighing in a vacuum, made by Bunge, of Hamburg. The observer is enabled to perform every part of the operation of weighing, stationed at a distance of six or eight meters. There is also a fine balance by Rupprecht, of Vienna.

In a second room, the instruments for determining the coefficients of expansion are mounted. This department is under the charge of Dr. Benoit. Both the method by immersion in a liquid, and the method of M. Fizeau, are employed. The latter method is described in the *Comptes Rendus* for 1866. It is also described in the Proceedings of the Royal Society of Nov. 30, 1866. I quote from the Proceedings: "In M. Fizeau's observations, he has availed himself of the possibility of forming Newton's rings with the monochromatic sodium light, when one of the interfering rays is 52.205 waves in advance of the other, a fact which, conjointly with M. Foucault, he announced in 1849. Using the length of a wave of sodium light 0.0005888 mm. [$0.00002318^{\text{in.}}$] as a standard of measure, the position of a ring being observable to within $\frac{1}{10}$ of the distance between two consecutive rings, the *variation* of the distances between the two surfaces producing Newton's rings can be measured within 0.00002944 mm. or $0.0001159^{\text{in.}}$" The results by this method, and those obtained by immersion in a liquid, are found to show a good agreement. The chief objection to its use is found in the fact that the method is only applicable to pieces of metal not much exceeding one centimeter in length. It must be assumed that the whole bar has the same coefficient as the small portion of it upon which the observation rests. In the discussion of the coefficient of the Mètre des Archives, it will be interesting to compare the value finally derived with the value found by M. Fizeau from a small section of the original bar which has been preserved in the Archives.

A third room of the building is devoted to the comparison of standards. This department is under the charge of Dr. Pernet. Here is mounted a fine comparator by Brunner Frères, built at a cost of 15,000 francs. A universal comparator by Starke, of Vienna, and costing 28,000 francs, will soon be in position. This apparatus will allow comparisons between standards two meters in length. There are also attachments for comparing subdivisions.

The Bureau has also a very perfect apparatus for determining the zero point and the boiling point of the thermometers employed in the comparisons. The *minimum* point of freezing is first found. To this minimum reading are applied the *variations* of the zero point, which are investigated for each thermometer. The thermometers are made by Baudin, of Paris. He makes two kinds. In one, the tubes are divided into a scale of equal parts; in the other, all the errors, except those of the zero and boiling points, are included in the graduations. The thermometers are read to single hundredths of a degree. It is the experience of Dr. Pernet that all the standard thermometers of Baudin will agree *inter se* within three hundredths of a degree.

I now pass to a consideration of the operations of the French Section, which are conducted in the building of the "École des Arts et Metiers," usually called the Conservatory.

Through the kindness of M. Tresca, who is the secretary of the Section, and, since the death of General Morin, the acting Director of the Conservatory, I was able, during a recent visit to Paris, to make a careful study of the entire operation of converting the "Mètre des Archives" into an equivalent line-meter. Notwithstanding the pressure of the official duties of M. Tresca, which were at the time of my visit especially great, owing to the illness and death of General Morin, the Director of the Conservatory, he gave me several hours each day, explaining in detail each step of the operation, from the melting of the platinum-iridium to the final comparison of the completed bar with the "Mètre des Archives." As the result of this somewhat critical study, I express the unqualified opinion that M. Tresca is entirely master of the problem. His methods and his results are at least beyond present criticism. In this I do not, however, include the method adopted of comparing end-measures with line-measures. This method, which is sometimes credited to Fizeau, and sometimes to Wild, seems to me to be radically defective.

To the end of a plate having the same thickness as the "Mètre des Archives," a strip of very thin platinum, terminating in a sharp point, is attached. The reflection of this point from the end of the bar gives the means of observing the point of contact without actually touching the surface. This method seems to be a necessity in this case, since a statute law forbids contact of any kind whatever. If it was not for this necessary limitation, a much better method could be employed. Still, it is the opinion of M. Tresca that the absolute error of the line prototype can be reduced below 1 $\mu = .001$ mm.

On account of the difficulties attending the transfer from an end-measure to a line-measure, M. Tresca has adopted the plan of transferring one line-meter with the utmost precision. This copy, which he calls his "working meter," has occupied his attention for several months. He is confident that every source of error has been eliminated. This line-meter will be the basis of the final standard, since the Commission will accept without question the definitive transfer offered by the French Section after the difficulties with respect to the question of material are removed. On this point there is no dispute. The real issue is this. The International Bureau ask of the French Section one or more bars of pure platinum, and several of platinum-iridium, with the definitive comparison with the "Mètre des Archives." The Commission contends that the bars already offered contain two per cent of iron, transferred through the process of drawing through dies. It contends also, that, by the process of drawing through dies, the bars remain in a state of strain. M. Tresca, on the other hand, contends that it is impossible that these criticisms can hold.

He admits that iron is transferred during the passage of the bars through the rollers, but this iron is extracted after each of the two hundred passages through the dies. M. Tresca also admits that the bars would be in a state of strain if left in the state in which they come through the dies, but he anneals them finally. As a proof of the correctness of this view, he gets exactly the same coefficient of expansion after each melting and casting of a given bar. Some bars have been melted and recast as many as ten times.

I heard but little said on the question of the alloy. The standard weights of the Bureau are made of platinum-iridium, and there seems to be no question about them.

The comparing rooms of the Conservatory are built as follows. First (*a*), there are the thick stone walls of the building; then (*b*), space filled with hay; (*c*), wooden walls; (*d*), space filled with cotton; (*e*), wooden walls, covered on the inside with paper. A copper room, entirely enclosing the comparators, is built in the central part of the enclosed space. The microscopes of the comparators are 90 centimeters in length. Only the micrometer screws project through the upper wall of the copper room, so that the heat of the body of the observer can produce but a very slight effect. The value of one division of the micrometer is 0.301 μ.

There are two rooms of the same form and dimensions. One contains the longitudinal comparator, on which the transfers are made, and on which a series of comparisons is also made directly after the transfers, the two bars remaining in the same relative positions. The other room contains the transverse comparator, with which the final and definitive comparisons are made. These comparators were built by M. Froment.

The transfers and comparisons are only made after the bars have been in position, and subject to the same temperature, for a period of forty-eight hours. All the transfers and comparisons are made in air. The tracings are made between one and two o'clock in the morning. Even then, the jar of passing vehicles is very perceptible. The disturbance is, however, strictly local. No matter how great the tremors, no permanent movement of the bars can be seen.

In the *exposé* of the condition of the labors of the French Section to Sept. 22, 1879, will be found a complete statement of the present state of this great work.

A third natural unit of length has been proposed in the length of a wave of light of given refrangibility. It is extremely doubtful, however, whether this unit will ever come into extensive use. In the present state of the measurements of wave-lengths, the total number in a meter is known with a far less degree of exactness than can be assigned to the comparison of different meters. Probably the wavelength of the line D is as well known as that of any other line of the spectrum, and yet the measures by different investigators show large discordances. The meter as determined by different observers shows the following errors when compared with the value given by Angström: —

Authority.	Error.
Fraunhofer	— 1.1 mm.
Ditscheiner	+ 0.8
Angström	+ 0.0
Van der Willigen	— 0.3
Mascart	— 1.0
Bernard	— 1.0

In the wave-length equation $\lambda = \epsilon \sin \theta$; ϵ represents the *mean* distance between the lines of the grating. A certain number of isolated errors may occur in the ruling, which may entirely escape detection under the spectroscope. For example, one of the provisional gratings, ruled with the machine built for the writer at the Waltham Watch Factory, has errors of spacing which are easily measurable, and yet the grating will show 7 and possibly 10 lines between b_1 and b_2, notwithstanding the fact that it is ruled upon commercial plate glass without finish.

Angström, in his investigations, found the distance between the extreme lines of the different gratings employed in terms of the Meter of the Archives; and the value of ϵ was found by dividing the entire distance by the number of spaces. I cannot find that he made any investigation, either of the accidental or the systematic errors of the gratings. I am aware that it is commonly asserted that it is impossible for systematic errors of appreciable magnitude to exist in a grating which shows the solar lines sharply defined; but there are many evidences that not only isolated accidental errors, but periodic errors of a small but measurable magnitude, are not incompatible with apparently perfect definition. Since the periodic error of a screw may undergo considerable variations in value through a change of temperature, especially if this change is abrupt and violent, it does not seem possible to overcome them entirely, except by a rigid investigation immediately preceding the ruling of a given grating, and by the application of the corrections derived, during the process of ruling. It is without doubt true that the *mean* interval between the ruled lines can be expressed with a far greater degree of accuracy than any given space can be measured under the microscope; but I believe it to be possible to measure the errors of lines widely separated when there is no evidence of their existence in the appearance of the solar lines. Even the best gratings which have thus far been produced show traces of systematic error when they are examined with monochromatic light.

Briefly, then, whenever the yard with its subdivisions is adopted as the measure of length, the unit to which all measures must be referred is the Bronze bar deposited in the "Strong Room" of Old Palace Yard, London, known as the "Imperial Yard No. 1." It has been shown that all attempts to express the length of the Imperial Yard in terms of a natural unit have been abandoned.

Wherever the metric system has been adopted, either by legal enactment or by actual use in the absence of definite legislation, the platinum end-measure meter, deposited in the Archives of Paris, is the only ultimate standard of reference. Since sixteen governments have entered into the Convention, which, through the International Commission, has decided that this particular bar at 0° Centigrade shall be the unit to which all measures of extension shall be referred, there is hardly a possibility that a different unit will ever be adopted. Great Britain is the only prominent government that has declined to enter the Convention. The two platinum meters which have hitherto been the standard of reference in that country are, however, no longer adapted to the purposes of exact measurement. Besides, even here the ultimate reference is the Meter of the Archives, through the equation determined by Kater and Arago in 1818.

The only exception to the entire abandonment of all attempts to refer the meter to a natural unit is the indirect determination by Clarke and others of the length of this unit expressed in the ten-millionth part of a quadrant of the earth's surface. With the highest possible respect for the work which has been done by Colonel Clarke, it does not seem likely that his value of this unit will ever become generally adopted. In the first place, his arc of the meridian does not follow the definition upon which the Meter of the Archives was founded. In the reference to any given unit, the standard of length determined must be ascertained with a greater degree of exactness than that attainable in the comparison of different copies of this standard. It is the experience of the writer that the error involved in the comparison of different meters need not exceed one millionth of a meter. It is not probable that an arc of a meridian of the earth's surface extending over 90° of latitude can be measured with sufficient exactness to warrant the assignment of this degree of accuracy to the aliquot part of this distance, which we call a meter.

In the United States, the particular yard which, previous to 1856, was taken as a standard, is the distance between the 27th and the 63d inch of a scale by Troughton. It has never had other than an indirect legal authority. It was never legalized by act of Congress. It was, however, adopted by the Treasury Department. The first standards distributed to the States by the authority and direction of Congress were copies of this particular bar.

In 1856, "Bronze bar No. 11" was presented by the British Board of Trade to the United States. It is standard at 61°.79 Fahrenheit. Since this date, all measures of length which are expressed in terms of the yard have been referred to this particular unit. This change, by whatever authority it was made, was one clearly demanded in the interest of science, and by the legalization of Bronze No. 1 as the Imperial Standard Yard, with which it had been most carefully compared; but I am not aware that it has ever been sanctioned by act of Congress.

For the metric system, the iron meter mentioned above has always been taken as the standard of reference. It has, however, no legal sanction. Neither in the case of the yard nor of the meter are comparisons usually made directly with the original standards.

The Saxton comparator consists of a brass bed-plate having V-shaped ways running the entire length. A slide carrying a microscope slides freely over these ways. A series of brass posts form a part of this bed, through which pass steel screws having conical ends, which have been tempered and polished. There are stops for the yard and for its subdivision into feet, and of one foot into inches. There are also stops for the meter and for its subdivision into decimeters, and of one decimeter into centimeters. By a very ingenious arrangement, the arm attached to the moving microscope plate can be brought into contact with any stop without loss of motion. The end stops for the yard and for the meter were many years ago set to correspond with "Bronze No. 11" at 58° nearly for the yard, and with the iron meter at 68° nearly. It is understood that the position of these stops with respect to the brass bed have never been changed. The standards which have been distributed since 1856 have been transferred from these distances at the temperatures at which they are standard. The yard in actual use at the Bureau of Weights and Measures, therefore, may be defined to be the distance between two steel stops attached to the bed of the Saxton comparator which corresponds to the length of "Bronze No. 11" at 58° nearly, and the meter may be defined to be the distance between two steel stops of the Saxton comparator which corresponds to the length of the iron meter corrected for the difference between its length at 32° and at 68° nearly. Recent comparisons indicate that these temperatures should be diminished, by a trifling amount, for the present distances between the stops, both for the yard and for the meter.

In May, 1878, by the kindness of Prof. J. E. Hilgard, Assistant in Charge of the United States Coast Survey, I was able to secure copies of both the yard and the meter, together with their subdivisions. On May 14, Dr. Clarke, who has charge of the standards, transferred the yard to a glass bar which I had previously prepared, and on the morning of May 17 the meter with its subdivisions was transferred to the same bar.

Upon my return to Cambridge, the relative relations between the subdivisions were investigated, with the following results. A positive sign indicates that the measured space is too short; a negative sign, that it is too long.

Subdivisions of the Yard.

	FEET.		INCHES.		INCHES.
No.	Corrections.	No.	Corrections.	No.	Corrections.
1	+ .00069 in.	1.	+ .00075 in.	7	— .00037 in.
2	— .00022	2.	— .00004	8	+ .00062
3	— .00047	3.	— .00031	9	+ .00009
		4.	+ .00024	10	— .00097
		5.	+ .00064	11	— .00020
		6.	— .00011	12	— .00036

Subdivisions of the Meter.

	DECIMETERS.		CENTIMETERS.
No.	Corrections.	No.	Corrections.
1	— .00080 cm.	1	— .00132 cm.
2	— .00018	2	+ .00021
3	— .00211	3	+ .00006
4	+ .00088	4	+ .00070
5	+ .00105	5	— .00070
6	+ .00142	6	+ .00003
7	+ .00041	7	+ .00100
8	— .00168	8	— .00019
9	— .00194	9	+ .00039
10	+ .00288	10	— .00016

These corrections involve the errors of transfer. No attempt was made to determine and apply the corrections due to the curvature of the ways upon which the slide carrying the microscope moves.

The subdivisions of the centimeter rest upon the authority of a centimeter subdivided into 100 equal parts by Brunner Frères, of Paris, for the office of the Coast Survey. By the permission of Professor Hilgard, I have made an extended series of comparisons between this unit and a centimeter derived from the screw of my own dividing engine. The comparisons were made by means of the comparator for short lengths, described in the American Microscopical Quarterly for April, 1879. The Brunner scale is divided on silver inlaid in brass. In my own scale, the graduations are upon glass.

Date.	Brunner. No. div. of comparator in 1 cm.	Thermometer. °	Rogers. No. div. of comparator in 1 cm.	B. — R. No. div.
1878, May 14	3149.02	70.4	3148.80	+ 0.22
15	3149.95	74.8	3150.11	— 0.16
16	3149.33	63.0	3149.53	— 0.20
16	3149.51	63.8	3149.53	— 0.02
16	3149.63	64.0	3149.21	+ 0.42
17	3149.66	72.1	3149.20	+ 0.46
17	3149.66	72.1	3149.40	+ 0.26
18	3149.18	62.0	3149.44	— 0.26
18	3149.58	68.3	3149.37	+ 0.21
18	3149.57	68.8	3149.37	+ 0.20
18	3149.42	70.0	3149.12	+ 0.30
19	3149.52	69.8	3149.17	+ 0.35
20	3149.30	71.3	3149.01	+ 0.29
21	3149.55	71.1	3149.09	+ 0.46
Means,	3149.49	68.7	3149.31	+ 0.18

In terms of the Brunner scale, therefore, my own unit is .000317 cm. × 0.18 = .000057 cm. too short. Subsequent investigations have shown that both units contain errors of considerable magnitude when compared with the hundredth part of the Coast Survey meter at 68°.

The following are the relative errors of each millimeter expressed in terms of the entire length of the centimeter: —

	Brunner. Correction.	Rogers. Correction.
Mm. No. 1	+ .000082 cm.	— .000016 cm.
2	— .000091	+ .000025
3	— .000079	+ .000012
4	— .000070	+ .000016
5	+ .000038	+ .000029
6	+ .000002	— .000016
7	+ .000056	— .000044
8	+ .000035	— .000022
9	— .000080	— .000006
10	+ .000105	+ .000013

Notwithstanding the fact that great advances have been made in the science of exact measurements since the construction of the normal standards now in use, there are several problems connected with this subject which require further investigation. It is a well-known fact, that, while the different results obtained in comparing two standards by one observer and with a given instrument usually indicate marvellous precision in the comparisons, a different observer with a different instrument will probably get results equally accordant *inter se*, but which nevertheless do not agree with those obtained by the first observer. Until all the sources of error involved in comparisons are investigated and eliminated, it will be useless to expect an agreement between different observers. Among the points which demand investigation, the following require special attention.

(*a.*) *The magnifying power of the microscopes employed, which is the best adapted to secure the greatest absolute accuracy in measurements.*

In all the earlier comparisons, microscopes of very low power were employed, varying from forty to sixty diameters. The International Commission, relying largely upon the recent investigations of Förster, have decided upon the low power of from forty to fifty diameters. M. Tresca, of the French section, on the contrary, is a firm believer in high powers; he prefers a power of about 400. The writer has had considerable experience on this subject, and always with results favorable to high powers. With a proper illumination, and with lines having smooth edges, a magnifying power of 900 can be used with great ease, even in the comparison of two meters upon a longitudinal comparator.

New microscopes have been recently attached to the microscopes of the meridian-circle of Harvard College Observatory. In order to be able to read the divisions of the circle, it was necessary to have one eye-piece with the same power as that furnished by the maker. A second eye-piece, giving nearly double the magnifying power, was attached to a swinging arm in such a manner that either eye-piece can be used at will. A sufficiently extended series of observations has now been made to justify the conclusion that the high-power eyepiece gives the most accurate results. Again, in the investigation of the errors of one of the circles of the instrument, a metal plate, having a graduated arc of 15°, is attached to the opposite circle under a one-inch objective, to which is attached the interior illuminator for viewing opaque objects, invented by Mr. R. B. Tolles, of Boston, in 1867, and also invented independently by M. Tresca, in 1871. The lines under this objective are sharply defined. The value of one division of the micrometer is only $0''.12$ against $1''.0$ for the regular microscopes. It is the experience of the writer, that it is quite as easy to make every reading fall within one division in one case as in the other. With the ordinary form of illumination, however, the advantage of a high power would not be so apparent.

The value of one division of the micrometer depends both on the magnifying power of the microscope and on the pitch of the micrometer-screw. Those who advocate a low magnifying power usually prefer a screw having a small pitch. Further observations are needed to determine the best relation between the pitch of the screw and the magnifying power of the objective. The following are the values of one revolution of a few of the micrometer-screws which have been used in the comparison of standards: —

Observer.	Value of 1 division. Inch.		Observer.	Value of 1 division. Inch.
Troughton, 1798,	.0001000		Chaney, 1880, new objectives,	.0000058
Kater, 1818,	.0000428		Rogers, 1880, Comparator, —	
Hassler, 1832,	.0001000	Microscope A.	With 1 in. objective,	.0000197
Baily, 1832,	.0000500		With $\frac{1}{2}$ in. objective,	.0000087
Baily, 1844,	.0000253		With $\frac{1}{4}$ in. objective,	.0000047
Bache, 1856, May,	.0000333		With $\frac{1}{4}$ in. and amplifier,	.0000028
Bache, 1856, October,	.0001000	Microscope B.	With 1 in. objective,	.0000079
Hilgard, Saxton Comparator,	.0001000		With $\frac{1}{2}$ in. objective,	.0000035
Clarke, 1866,	.0000286		With $\frac{1}{4}$ in. objective,	.0000019
Chaney, 1880, old objectives,	.0000319		With $\frac{1}{4}$ in. and amplifier,	.0000011
Tresca, 1880, 0.301 μ =	.0000118		Internat. Comm., 1.0 μ =	.0000394

The writer is inclined to the opinion that one can measure with *certainty* only what one can see.

(*b.*) *The best method of illumination for opaque objects.*

I cannot better illustrate the necessity for a proper illumination in making exact measurements, than by saying that I have been obliged to reject a series of observations extending over a period of four months, for the simple reason that I finally discovered that during all this time I have never once seen the actual lines ruled, but only their image. I used a parabolic reflector, giving a beautiful *white* line on a black background. The lines were traced upon a steel surface nickel-plated, their width being about one ten-thousandth of an inch. Investigation showed that the positions of the lines could be changed by an amount more than half their width, by shifting the position of the parabolic reflector.

The method of illumination employed by Baily and Sheepshanks seems to me radically defective. With the microscopes used by Sheepshanks I found myself unable to separate lines ruled on a polished steel plate, though separated by an interval of only one-thousandth of a centimeter. As already stated, I have used with great satisfaction the form of illumination described by Mr. Tolles in the Annual of Scientific Discovery for 1866–67. I found this form of illumination in use by M. Tresca since 1871. It has also been since described as an original invention by Professor Wild. Troughton and Simms also constructed microscopes with the same method of illumination as early as 1869, at the instance of Mr. Warner, a retired gentleman residing at Sussex Place, Brighton. According to the present evidence, the priority of publication must be assigned to Mr. Tolles. M. Tresca was without doubt the first to make an actual application of the method to exact measurements. The reader who is interested in pursuing this subject farther will find a full description of the method in the number of the Journal of the Royal Microscopical Society for August of the current year. It is sufficient to say here, that, as none of the light is lost by the reflection, it is easy to get all, and even more, than is needed. Diffused daylight falling upon the plane face of the prism inserted between the two front lenses affords an abundance of light for the most delicate tracings. With a one-inch objective of the form recently constructed by Mr. Tolles, lines 30,000 to the inch, ruled on a polished steel surface, are resolved with the greatest ease.

(*c.*) *The method of support which is best adapted to neutralize the effect of the flexure of the bars upon which the graduations are traced.*

In all the early measurements the standards were placed upon a planed surface of wood. Troughton, in comparing Shuckburgh's scale, fastened it to a bed of mahogany by means of three screws. Kater was the first to discover the variations due to the flexure of the bars on which the graduations were traced. He was also the first to suggest a neutral plane, in which the effect of flexure upon the length would be zero. At first he located this neutral plane in the middle of the bar, but from subsequent investigations he concluded that it was not quite one third of the thickness of the bar below the graduated surface. He reached the following conclusions*: —

(1.) "That in a standard of lineal measure, traced upon the surface of a bar, an error arises from the thickness of the bar when it is placed upon a table, the surface of which is not plane."

(2.) "That this error in bars of the same material and of unequal thickness is within certain limits as the thickness of the bar, and depends upon the extension of that surface of the bar which becomes convex, and the compression of the surface which is concave."

(3.) "That the error to which the scale is liable from this cause is directly as the versed sine of the curvature of the surface upon which the scale is placed."

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* Phil. Trans., 1830.

(4.) "That this error very far exceeds that which would arise from the difference of length between the arc and its chord, under similar circumstances; so much so, that the sum of the errors from this cause in a bar one inch thick, with a versed sine of not one-hundredth of an inch, is nearly one-thousandth of an inch; whilst double the distance between the chord and the arc is not one fifty-thousandth."

In the early observations of Kater, he used a wood surface for a support, but later he seems to have preferred a marble slab, which, however, was not planed.

In 1844 Sir George Airy showed that, if n represents the number of supports of a bar, the distance between the supports should be

$$\frac{\text{Length of bar}}{\sqrt{n^2-1}}$$

in order to neutralize the effect of the flexure. Thus, in the case of the yard, if the defining lines are near the ends of the bar, each support should be placed 10.39 inches from the centre, and in the case of the meter they should be placed 28.87 centimeters from the centre.

This general form of support was used by Mr. Sheepshanks in all of his observations, and it is the form which is ordinarily employed at the present time. In the construction of the National Standards it was considered important that the bars should be supported at numerous points in order that they should be exposed to as little strain as possible. The particular form of support finally adopted will appear from the following description by Sir George Airy, to whose suggestion it is due.

"Great facility is given to the arrangements for supporting a bar with definite pressures applied at special points, by the use of levers. Thus, if any portion of the bar rest upon two rollers which are placed at the ends of a lever, and if the fulcrum of this lever (whether movable or not) be in its centre, the pressure upwards produced by these rollers will necessarily be equal. If there be another such lever, and if the fulcrum of this and the former be upon the extremities of a third lever, and if its fulcrum be at its centre, then the pressures upward produced by the four rollers will be equal. By this arrangement of the rollers and levers, one half of the bar may be supported. If another similar system be applied to support the other half of the bar, the pressures produced by its four rollers will also be equal among themselves; and if the bar be laid symmetrically upon them, all the individual pressures will be equal." *

Mr. Baily decided upon the adoption of eight rollers for the support of the National Standards, requiring for the distance between each roller and the one next adjacent $\frac{36}{\sqrt{63}}$ inches = 4.54 inches. As a further precaution, the defining lines were traced upon gold plugs inserted in wells sunk to the plane of the neutral axis. This form of support is the one now employed in the Standards Office. The reader who desires to pursue this subject will find elaborate discussions by Bessel and by Clarke.

At the International Bureau only two supports are used, the distance between them being determined by Bessel's formula. At the Conservatory, the bars are placed directly upon a plane surface, which is nearly in the neutral axis of the support itself.

In 1876 Professor Wild proposed a form of support which seems to leave very little to be desired. The bars are placed *one above the other with the graduations in the same vertical plane.* Here we have conditions quite unlike those which occur with bars supported in the way already described, and under which it would seem that no flexure can occur which will affect the distance between the defining lines. This method, therefore, affords a rigorous test of the flexure formulæ of Bessel and Airy. It appears from the discussion of Professor Wild, that, while the mean effect of observed flexure upon the relative lengths of the separate decimeters of the same bar agrees nearly with the mean computed value, the numerical mean of the differences between the observed and the computed effects is no less than 0.0004 mm., or nearly one half of the whole mean effect. On the other hand, a rigorous comparison instituted by Clarke showed a substantial agreement between the computed and the observed effects of flexure. It is evident, therefore, that this subject requires further investigation. In the case where eight, or even four rollers are employed, it is mechanically impossible to make them so that planes tangent to each roller shall fall in a common plane, which shall be parallel with the plane of the defining lines. Unless this takes place, the upward pressures will not be equally distributed, and the formulæ will not hold. In the case of bars of the Tresca form, I am compelled to admit that, at least with a bar of copper, attention must be paid to the form of the support.

In the comparator which has been constructed from designs furnished by myself, I have dealt with the question of supports in the following way. The bed of the comparator is made of cast-iron, and is sixty inches long by fourteen inches wide. It has an extreme depth of two inches. In the centre, V-shaped ways run the entire length of the bed, upon which a plate carrying the microscopes, slides. The bed has at one end the means of bringing it into a horizontal plane, and at intermediate points screws for taking up the flexure. For this purpose, free vertical bolts, pressed upwards by means of levers and controlled by weights, are without doubt better than screws.

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* Astron. Trans., xv. 157, &c.

It is now necessary to provide for the movement of the microscope slide in a horizontal plane. This is accomplished in the following way. A shallow dish of mercury is placed upon the bed of the comparator, extending along its entire length. An arm projects from the microscope plate, to which is attached a plate sliding between guides, and carried by a micrometer-screw. To the lower part of this slide a platinum point is attached. One wire of a battery having a single cell is attached to the platinum. Another is placed in contact with the mercury. A sounder is placed in the circuit. The microscope plate is moved to one end, and the platinum point is brought into contact with the mercury, the contact being indicated by the " click" of the magnet. The slide is then moved to the other end, which is elevated or depressed by means of the adjusting screws, till the platinum point again makes contact with the mercury. After one, or at the most two trials, it will be found that the two ends of the bed-plate are in the same horizontal plane. The microscope plate is now set at the middle of the comparator, and the amount of the flexure is measured with the micrometer-screw. After about one third of the measured amount has been taken up by means of the flexure screws, the entire operation is repeated. In this way I find that the microscope plate can be made to move sensibly in a true plane. In practice, it is found that almost equally good results can be obtained by directly observing the surface of the mercury, with an objective of pretty high power, using the interior illuminator. The surface of the mercury admits of nearly as sharp a focus as the surface of a metal bar. Good results have also been obtained by dropping fine threads of spun glass upon the surface of the mercury. This method is rather more convenient than the method by contacts, but the latter admits of somewhat greater precision. From a limited number of trials, I conclude that contacts can be made with a probable error of a single contact not exceeding .00002 inch.

Knowing that the microscope plate moves in a true plane, the surface of the bed-plate on either side can be brought to a plane which shall be parallel with that through which the microscope moves, by working it down till every part remains in the same focus. The bars to be compared are placed directly upon the surface thus prepared.

(*d.*) *The form and material of a bar which is best adapted to fulfil all the conditions which are essential to success in comparisons extending over a long period of time.*

In general, the form of a standard bar should be the same as that with which it is to be compared. For example, if one desires a standard yard which is to be compared with "Bronze 11," at Washington, the bar should be made of Baily's metal, and should be one inch square and about thirty-eight inches long. Kater preferred a thin bar. A width of one centimeter with a depth of three centimeters will be found to yield good results when the bar is placed upon a flat surface. Of all the forms proposed, that of M. Tresca, which has been adopted by the International Commission, seems to me the best designed to overcome all the difficulties of the problem. It is convenient to handle; it retains its form under its own weight, and quickly answers to a given change of temperature. I have had the pleasure of using a bar of this form for several months with the most satisfactory results. I began with considerable prejudice against it, influenced to some degree by a remark made by Professor Wild concerning it. It is undoubtedly somewhat difficult to manufacture, and will probably be found to be rather costly; but these are the only serious objections that can be urged against it. I express the opinion that it is well adapted to scientific work of the highest order. For use with my own bar, I have had constructed a special objective provided with an interior illuminator. The working distance is just sufficient to allow the passage of the bar under it.

(*e.*) *The investigation of the error due to the horizontal curvature of the ways of a longitudinal comparator.*

If the microscopes are stationary, and the bars to be compared are brought in succession under them, the curvature of the ways will produce no effect; but when the relation between the separate subdivisions of the given unit are to be investigated, or when a given length is to be transferred from one bar to another, the error arising from the curvature of the ways cannot be neglected. The comparisons of Troughton, of Hassler, and of Bache are subject to this class of errors, though of course it might have happened that in each case the curvature of the ways was insensible. I cannot find that any observations were made to determine the amount of the curvature. By reversing the position of the bars to be compared, the error due to curvature will be eliminated in proportion to the ratio between the length of the chords described by the microscope for the two positions of the bars.

The necessity for taking into account the error due to curvature in any given case will clearly appear from the following provisional investigation of its magnitude in my own comparator: —

O A = —3.8 cm.
O B = +3.8
O C = +23.8
O D = +26.8
O E = +30.6

A ——— *a*
O ——— *o*
B ——— *b*

C ——— *c*
D ——— *d*
E ——— *e*

A steel meter by Froment was placed in the constant position O *o*. A copper meter of the new form by Tresca was placed successively in the positions A *a*, B *b*, C *c*, D *d*, and E *e*. The microscopes were attached firmly to the plate, moving freely upon the ways of the bed-plate, each having its own adjustment for focus. Microscope B was adjusted for coincidence with the end line of the bar O *o* at O, and microscope A was at the same time adjusted for coincidence with the end line of bar A *a* at A. The plate was then moved along the ways until microscope B was adjusted on the terminal line at *o*, and for this position the micrometer of microscope A was read. Since the two microscopes remain in the same position with respect to each other, the difference between the two readings of A will indicate the difference between the length of the bars. By reading B for the positions O *o*, — A being a constant, — the relation between the two bars will be expressed in terms of the micrometer of B also.

Differences in the Apparent Length of the Bars compared, varying with the Position of Bar A *with respect to Bar* B.

		Divisions of Micrometer. From Microscope A.		Divisions of Micrometer. From Microscope B.	
O–A	=	—17.7	= —9.0 μ	—102.0	= —9.1 μ
O–B		+13.3	+6.7	+80.3	+7.1
O–C		+83.3	+42.1	+474.0	+42.2
O–D		+96.4	+48.8	+553.7	+49.3
O–E		+112 4	+56.9	+635.0	+56.5

For the radius of curvature, we have approximately,

$$.00658 : 34.4 = 100 : x.$$

$$\therefore\ x + = 5228 \text{ meters.}$$

It is apparent, therefore, that, though the radius of curvature exceeds a distance of three miles, a correction of 3.8 divisions must be applied to the reading of microscope A, and of 21.4 divisions to the reading of microscope B, for each centimeter of the distance between the two bars.

The radius of curvature can also be found in the following way. If a tracing apparatus is attached to the microscope plate, a line traced upon the plane surface of a bar, by the motion of the plate from one end of the bed-plate to the other, will have the curvature due to the distance of the ruling diamond from the centre of the slide. If the bar is reversed, and a second line is drawn upon the same surface as nearly parallel to the first line as possible, then the ver-sin of the curvature will be equal to one half the difference between the distance of the lines at the middle point and the half sum of the distances at the two ends.

Finally, the deviation of the microscope plate both from a horizontal and from a vertical plane can be detected by means of a telescope mounted upon the sliding plate. If the telescope is pointed either at the cross wires of a collimator, or at a distant object, and the point remains fixed with respect to the cross wires of the telescope during the motion of the slide from one end of the comparator to the other, it may be assumed that it moves in an invariable plane. The longitudinal comparator at the Conservatory is provided with an attachment of this kind.

(*f.*) *The relative advantages of comparisons in air and comparisons in a liquid.*

Air temperatures are employed both at the Conservatory and at the International Bureau. At the Conservatory, the bars to be compared remain at a constant air temperature for forty-eight hours before the comparisons are made. The arrangements for maintaining a constant temperature are most admirable and effective. The means of controlling the temperature employed at the International Bureau are somewhat different, but they give most excellent results. Nevertheless, it is yet an an open question whether the absolute relation between two bars compared in air at a given temperature can be made to agree with the absolute relation determined by submerging them in a liquid at the same temperature. The writer has met with many difficulties in this direction.

(*g.*) *The variation of the absolute length of a bar by a change in its molecular structure or otherwise.*

There are some evidences that certain standards have undergone a change of length since their original construction, but in no case does the evidence seem to me to be conclusive. It is understood that Colonel Clarke finds a well-defined change in some of the standards originally measured in 1842–55. The platinum meters of the Royal Society present some evidences of a change of length *inter se.*

According to the Fifth Report of the Standards Commission (Appendix), we have the Royal Society end-meter equals: —

Royal Society line-meter +0.01759 mm. (Arago, 1818.)
" " +0.01881 (Kater, 1818.)
" " +0.00940 (Baily, 1835.)
" " +0.00837 (Standard's Office, 1869.)

We have here an appearance of a change. In deciding whether it is a real or an apparent change, it should be remembered that in 1818 there was no defining cross-line on the line-meter, and that there is no existing data by which the accuracy of the constants of the contact pieces used with the end-meter can be estimated.

The Russian standard of length used in the geodetic surveys previous to the work done by Prazmowski and Wagner presents the most authentic instance of a well-defined change of length which has come under my notice. This bar is made of iron, and has a length of seven feet. I am not certain whether it was forged or drawn through dies. Conical end-pieces of tempered steel were inserted in each end. In the course of two or three years, this bar was transported a distance exceeding 8,000 miles, being supported in a packing of feathers. At the end of this time it was found by Prazmowski to be one thirteenth of a line, or about .006 inch shorter than at the commencement of the expedition, the two sets of comparisons having been made at the same temperature.

If it can be established that no permanent flexure of the bar took place, we have here an authenticated instance of an actual change of length. Upon the discovery of this change, from whatever cause produced, a new standard was constructed, also made of iron. It was allowed to anneal for eight days after being forged. It is understood that no change of length has ever been detected in this bar. Against this somewhat doubtful evidence we have the positive evidence by Chisholm, that Bronze No. 6 showed no evidence of a change in length in 14 years, and of Baeyer that the precise mean length of Bessel's standard bars at 13° Reaumur had not altered in the 20 years from 1834 to 1854. The change in the mean length is to be distinguished from a change in the coefficient of expansion. The evidence of Baeyer seems tolerably conclusive that bars of iron and zinc are liable to suffer a change in their coefficients of expansion, either by an actual change in their molecular structure or by the action of external causes.

The continuous measurements of the Conservatory and of the International Bureau will, in the course of the next decade, furnish data which will add much to our knowledge of this subject.

(*h.*) *The law which governs the expansion of bars of different materials, and having different masses, under a varying temperature.*

It is well known that Mr. Sheepshanks, in the comparison of "Bronze bar 28 with Cast-steel bar D," near the close of his labors on the National Standards, found deviations which he could not explain. The importance of this matter justifies me in quoting in full the statement of Sir George Airy in his account of the construction of the national standards.

"I proceed now to allude to a discordance which was a source of great anxiety to Mr. Sheepshanks.

"In April, 1855, Mr. Sheepshanks was engaged in measuring the bar Cast-steel D. By comparisons with four iron bars, (as stated in the table above,) whose results agreed very closely, the excess of Cast-steel D above Bronze 28 was found to be —$3^d.61$. But a direct comparison of Cast-steel D with Bronze 28 immediately preceding had given —$0^d.46$. This comparison was made at the temperature 45°.54, or 16°.46 below the standard temperature. A trifling error of expansion might account for part of the discordance, and the ordinary errors of observation might account for part. But in the opinion of Mr. Sheepshanks, though the whole discordance scarcely exceeded the effect of the thermometric expansion of Bronze 28 for 0°.3 Fahrenheit, it was impossible so to explain away the whole or a large part of it; and he was convinced that Bronze 28 had sensibly shortened. And so deeply and so painfully was this impression fixed in his mind, that he actually contemplated the rejection of all the results which had cost so many years of labor, and the commencing the work *de novo*."

Mr. Sheepshanks first disproved, by observation, the first conjecture on the possible cause of the apparent change; viz. "that Bronze 28, still covered with gold-beater's skin and cement (as in the earlier comparisons), might have been so constrained by that covering that it could not shrink down to its natural length; but that in the last comparisons with Cast-steel D, when that covering had been removed, it had contracted itself."

He then compared Bronze 28 with twenty-seven different bronze bars, and by comparing the old and the new measures found, with only one exception, a very close accordance. Mr. Airy concludes this part of his report as follows: —

"First, there is no evidence whatever of a general preponderance of excess from the New Measures above the excess from the Old Measures; the signs + and — being intermixed, in the differences, in all possible ways, and the mean of the whole being less than $0^d.50$. Secondly, the only instance which fairly supports the conclusion deduced from Cast-steel D is the first of all, namely, Bronze 12. Cast-steel D was compared on April 13, 14, and 16; Bronze 12, on April 26, 27, 28, and May 1; Bronze 39 (the next), on April 30. The conclusion, I think, is inevitable, that Bronze 28 really was shortened at the beginning of April; that it recovered its exact length before April 30; but that this recovery took place with some fluctuations, so that on May 1 it was subject to nearly the same error as before. Bronze 21, observed June 26, exhibits a similar discordance. What circumstances can have produced these changes, or how far the later fluctuations are apparent rather than real, I am wholly unable to conjecture."

I believe that the explanation of the phenomena observed by Mr. Sheepshanks will be found to fall under the following: —

First, *two bars of different materials, having different shapes and different masses, have a variable coefficient of expansion with respect to each other, which is a function of the time of exposure to a given temperature.*

Second, *the more violent the change of temperature, the greater will be the variation in the length of the bars before they assume their normal condition under a constant temperature.*

All of the bronze bars had the same mass. The iron bars and the steel bars, on the other hand, not only had a different mass, but they were subject to a different degree of specific heat. Their conductive power was also different. They also had a different absorptive power. The difficulty with the observations of Mr. Sheepshanks was, that they were not sufficiently continuous. They did not extend over a sufficient length of time to enable him to discover the slow changes which were going on in the length of the bars through the heat already absorbed, and *which was not indicated by his thermometers.*

This paper has been already extended so far beyond the limits proposed that it is inexpedient to give even a *résumé* of all the observations which I have made bearing upon this point. It will be seen from the following brief account, that it is absolutely necessary to investigate the performance of two standards which are to be compared under the action of a varying temperature, in order to decide how long they must remain at nearly a constant temperature before comparisons can be safely made. The comparing-room is a small triangular space partitioned off from the cellar of the west wing of the Observatory. It has two windows, one facing south and the other west. All the heat of a furnace can be turned into the room through a pipe entering it near the ceiling. The comparator is mounted on brick piers insulated from the building. The temperature of the room can be considerably reduced by means of two refrigerators, supported near the ceiling. Centigrade thermometer No. 1 is imbedded in the base of the comparator, and packed with iron filings taken from the bed-plate. Centigrade thermometer No. 2 is suspended by a fine wire about half-way between the point where the heat from the furnace enters the room and the upper surface of the comparator.

Series I. consists of partial records of a comparison of a line-measure steel yard by Troughton and Simms with a yard laid off on platinum-iridium plugs inserted in the bed of the comparator. Series II. consists of partial records of a comparison between two line-measure steel bars, of which one is nickel-plated, the graduations being upon the nickel surface. These two bars were made at the same time, and of the same material. The bars have the following dimensions: —

	Length.	Breadth.	Depth.
Nickel-plated bar,	40.4 in.	0.5 in.	2.0 in.
Steel bar,	39.37	0.6	1.6

A thin vertical lamina of platinum is inserted in the shorter bar. In other respects, it differs from the longer bar only in its dimensions, and in not being nickel-plated.

After the bars were placed in position, they were not disturbed till the close of the observations. Whatever changes took place, therefore, were due entirely to the action of temperature upon the bars. In Series I. the value of one division of the micrometer employed was .0000035 inch. In Series II. it was .0000197 inch, as stated on page 291.

SERIES I.

	Time. h.	m.	Thermometers. No. 1.	No. 2.	Diff. in Length in Divisions of Micrometer.
1880, Mar. 22,	9	45 A.M.	5.7	—	+35.6
	9	48	5.9	—	+35.3
	9	50	6.4	—	+40.7

Closed windows, and turned on heat.

	Time.			Thermometers.		Diff. in Length in Divisions of Micrometer.
	h.	m.		No. 1.	No. 2.	
1880, Mar. 22,	9	54	A.M.	6.7	—	+54.2
	9	57		6.8	—	+69.1
	9	59		7.6	—	+77.0
	10	2		8.3	—	+97.8
	10	4		9.0	—	+116.3
	10	7		9.4	—	+136.4
	10	11		10.0	16.7	+158.1
	10	15		11.0	19.4	+197.4
	10	18		11.4	20.6	+218.5
	10	20		11.7	21.1	+238.5
	10	23		12.2	22.2	+260.0
	10	26		12.5	22.5	+280.8
	10	28		12.8	23.3	+300.8
	10	30		13.1	23.9	+328.8
	10	32		13.4	24.0	+347.1
	10	35		13.9	24.9	+369.6
	10	38		14.2	25.0	+389.8
	10	41		14.7	25.6	+404.0
	10	43		15.1	26.2	+423.8
	10	46		15.3	26.7	+443.0
	12	0		20.3	33.3	+660.1
	0	4	P.M.	20.4	33.2	+659.7
	0	46		25.5	34.4	+615.9
	0	49		25.7	—	+611.7
	1	46		29.2	35.6	+550.6
	1	49		28.9	35.6	+537.9
	2	5		29.6	—	+499.9
	5	53		38.8	—	+50.2
	6	1		38.3	—	+31.1
	6	3		38.2	—	+23.0
	6	7		38.3	—	17.0
	7	18		38.8	—	+2.4
	7	50		38.8	—	—1.6
	8	21		38.8	—	—1.2
	8	37		38.8	—	—4.8
	8	39		38.8	—	—9.1
Mar. 23,	6	28	A.M.	5.0	—	—167.1
	6	30		5.4	—	—165.8
	6	50		7.3	—	—132.2

	Time.			Thermometers.		Diff. in Length in Divisions of Micrometer.
	h.	m.		No. 1.	No. 2.	
1880, Mar. 23,	9	7	A.M.	9.9	—	+62.9
	10	47		11.6	—	+19.4
	7	58	P.M.	19.0	—	—86.5
	8	1		19.1	—	—81.8
Mar. 24,	6	44	A.M.	17.5	—	—147.2
	7	51		17.2	—	—150.4
	8	54		17.2	—	—149.9
	8	57		17.2	—	—151.8

SERIES II.

	Time.			Thermometers.		Diff. in Length in Divisions of Micrometer.
	h.	m.		No. 1.	No. 2.	
1880, April 11,	4	33	P.M.	47.7	51.1	—297.3
	4	35		47.7	—	—296.5
	4	37		47.7	50.8	—295.0
	Shut off heat, and opened windows.					
	4	43		—	—	—288.1
	4	47		46.8	23.0	—327.0
	4	49		45.0	19.2	—420.3
	4	52		44.4	19.1	—464.1
	4	55		43.8	18.9	—535.7
	4	59		42.8	19.0	—580.7
	5	1		41.0	19.0	—638.1
	5	5		40.2	19.0	—669.5
	5	9		—	—	—702.1
	5	10		—	—	—707.7
	5	13		38.3	20.0	—716.0
	5	27		36.0	21.0	—742.5
	5	32		—	—	—746.7
	5	36		34.0	20.3	—741.2
	5	41		—	—	—730.7
	5	46		33.6	19.2	—694.5
	7	35		24.1	17.8	—497.1
	10	5		8.2	4.4	—453.9
April 12.	Windows open all night.					
	6	46	A.M.	1.2	1.3	—374.3
	7	18		2.7	1.1	—369.6
	8	7		1.8	1.7	—363.9
	2	34	P.M.	4.6	6.2	—311.6

	Time. h.	m.	Thermometers. No. 1.	No. 2.	Diff. in Length in Divisions of Micrometer.
1880, April 12,	4	46 P.M.	6.7	9.8	—283.0
	8	30	8.2	7.9	—360.7
	8	33	8.2	7.9	—364.9
	10	20	7.2	6.3	—386.5
	10	35	7.1	6.9	—376.5
	10	56	7.1	7.0	—376.2
	11	25	6.6	6.1	—378.0
	12	16	6.2	5.2	—372.0
April 13.	Windows open all night.				
	6	53 A.M.	3.1	3.9	—329.4
	6	59	3.1	3.9	—326.9
	8	23	4.1	6.0	—302.0
	8	26	4.1	6.0	—296.1
	8	33	4.3	6.9	—290.2
	9	13	5.2	8.2	—266.7
	9	15	5.2	8.2	—268.5
	10	6	6.9	10.2	—243.5
	10	8	6.9	10.2	—246.3
	11	37	9.7	13.0	—247.9

If the changes of temperature are gradual, the two steel bars will reach a state of rest under a constant temperature in about twelve hours; but if they are subjected to an abrupt and violent change of temperature, it is not safe to make the comparisons till after the lapse of from forty-eight to sixty hours.

Baily's comparisons of the yard of the Royal Society certainly show traces of an error of the kind I have here described. On the other hand, Clarke's comparisons do not show any marked evidence of their existence. But in the former case, the comparing-room was not well adapted to the maintenance of a steady temperature, while in the latter it appears to have been admirably constructed. As observations are still being made with the apparatus at Southampton, it would be interesting to see if results corresponding in a general way with those I have found could be obtained from a special series of comparisons arranged for this specific investigation.

An investigation of the character which I have indicated is especially necessary in the measurement of base lines in which the standard unit is necessarily subject to greater and more rapid variations of temperature than take place in a well arranged and protected comparing-room. Still further observations are neccssary in order to determine whether end-measures behave in the same way as line-measures under a varying temperature.

(*i.*) *The exact relation between the length of the Imperial Yard and the Meter of the Archives.*

The Imperial Yard has never been directly compared with the Meter of the Archives. Our knowledge of the value of the equation between these standard units depends solely upon, —

First. The indirect comparison of the platinum meters of the Royal Society with the Meter of the Archives by Kater, Baily, Clarke, and Chisholm. All these determinations rest upon the relation of the Royal Society platinum meters to the Meter of the Archives given by Arago in 1818.

Second. The comparison of the iron meter of the United States Coast Survey with Troughton's scale, by Hassler (27$^{\text{in.}}$—63$^{\text{in.}}$) in 1832.

The following are the relations given by the authorities named above. I have subtracted .00087 inch from the value given by Hassler, this being the amount by which the yard on Troughton's scale exceeds the length of "Bronze 11" when reduced to 62°.

Kater, 1818,	"Mètre à Traits"	= 39.37076 inches.
	"Mètre à Bouts"	39.37081
Kater, 1820,	Dolland's Scale	39.37045
Baily, 1836,	"Mètre à Traits"	39.36968
	"Mètre à Bouts"	39.36937
Clarke, 1866,	"Mètre à Traits"	39.37048
Chisholm, 1870,	Standard Meter on Baily's metal	39.37112
Hassler, 1832,	Original Iron Meter	39.38005

Mr. Hassler compared several other standards with Troughton's yard, but they are not included in the above table; first, because, with the exception of the brass meter by Lenoir, there is no evidence that the standards compared are authentic copies of the original; and, second, because the certificate of the meter by Lenoir refers to the Meter of the Observatory, and not to the Meter of the Archives.

It is obvious from this table, that our present knowledge of the relation between the length of the yard and the meter is subject to great uncertainty. In deciding upon the weight which should be assigned to any particular value, it is to be remembered, —

(*a.*) That in the comparisons by Kater, Baily, Clarke, and Chisholm, the constant relation given by Arago is subject to great doubt.

(*b*.) That the meters of the Royal Society do not admit of exact measurements in their present state.

(*c*.) That Kater has himself declared his own determination to be erroneous.

(*d*.) That the values given by Baily are expressed in terms of the Scale of the Astronomical Society, which for 39.37 inches is .00076 inch *longer* than the Imperial Yard.

(*e*.) That the value given by Mr. Chisholm involves the errors of transfer to the bar of Baily's metal.

(*f*.) That the lines on the bar of Baily's metal do not admit of great precision in comparisons with moderately high powers.

(*g*.) That the original iron meter has never been compared with the Meter of the Archives, but only with the Meter of the Conservatory.

In view of these facts, it does not seem too much to say that it is at present impossible to assign any value to the equation between the yard and the meter, which is not liable to an error as great as .005 inch.

Finally, the confusion with regard to this relation has become so great, that, by an act of Parliament passed in 1878, the relation found by Kater was declared to be the legal relation, without regard to the various determinations which have since been made.

I will close this article with an abstract from the Philosophical Transactions for 1798, p. 180, giving the errors of the six-inch spaces of Troughton's scale as determined by Shuckburgh. As this scale is understood to be still in existence, it would be interesting to have a remeasurement of these spaces with a comparator of modern construction. I know of no modern graduations which have a much greater apparent accuracy than is here indicated.

Inches.	Error, or Difference from the Mean.
0 to 6	+.00012 inch.
6 to 12	+.00000
12 to 18	+.00007
18 to 24	—.00013
24 to 30	—.00006
30 to 36	+.00020
36 to 42	—.00033
42 to 48	+.00007
48 to 54	—.00003
54 to 60	+.00010

HARVARD COLLEGE OBSERVATORY, April 14, 1880.

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